

Interactive comment on “Microbial dormancy and its impacts on Arctic terrestrial ecosystem carbon budget” by Junrong Zha and Qianlai Zhuang

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General comments

Although no questions, hypotheses or goals are explicitly described in this manuscript, the authors implicitly addressed interesting and novel questions e.g. what is the role of microbial dormancy in the carbon budget of the Arctic terrestrial ecosystems? How do projections of net primary production (NPP), soil heterotrophic respiration (RH) and net ecosystem production (NEP) change when considering microbial dormancy in soil biogeochemistry models? For the most part (see specific comments about mathematical formulations), the methods to address these implicit questions are valid and well explained. The authors found that the fit between observations of NPP, RH, and NEP and

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model predictions with the soil biogeochemistry model MIC-TEM, is better when microbial dormancy is considered (i.e. MIC-TEM-dormancy). Moreover, predictions from MIC-TEM and MIC-TEM-dormancy varied notably across seasons and under RCP 2.6 and 8.5 scenarios, suggesting that microbial dormancy plays an important role in the ways soils from arctic terrestrial ecosystems respond to seasonal and global warming. For example, during winter MIC-TEM does not account for maintenance respiration of microbes in dormant state, while MIC-TEM-dormancy does. This leads to predictions of larger winter RH by MIC-TEM-dormancy than by MIC-TEM. In contrast, during summer MIC-TEM assumes that all soil microbes are metabolically active while MIC-TEM-dormancy acknowledges that (as empirical evidence show) in natural soils a large proportion of microbes remain in dormant metabolic state, even in summer. This because of limitations in factors other than temperature, such as moisture and nutrients. Therefore, MIC-TEM predicts (likely unrealistically) larger summer RH than MIC-TEM-dormancy. Although overall good, this work suffers from two main weaknesses: 1. The authors almost completely ignored in their discussion previous efforts to include microbial dormancy in soil biogeochemistry models. It would be interesting, for example, to know what is the contribution of this work to those previous efforts; and what are the consistencies/inconsistencies of their findings in comparison with results from other “dormancy models”. 2. The language. There are so many small language issues that make the reading of this manuscript hard. The authors jump between active and passive voice, and between past and present tense. They explain abbreviations over and over again (RH was defined four times!). They show some data in the results and then show it again in the discussion. I added many small comments that in my opinion could help to address these and other similar issues.

Specific comments

L93: consider replacing “to remedy the inadequate representation of soil decomposition process” with something like: to represent decomposition in ways that include important microbial processes that were previously ignored

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L100: ...models (Wieder et al., 2015)

L107: “a fraction of number of microbes, likely below 50% of live microbes, in natural soils” do you mean: a fraction, likely below 50%, of metabolically active microbes in natural soils ?

L112: ...biomass (Wieder et al., 2015)

L111: use total rather than active microbial biomass as indicator of microbial activities, which could...

L114: modeling microbial dormancy in the Arctic is important not only because arctic ecosystems are N-limited (many other places in the world are N-limited as well (Wang et al., 2010. BG), but also (and maybe more importantly) because of the marked seasonality (i.e. activity/dormancy cycles) and the above-global-average warming happening in those latitudes (which could increase the abundance/proportion of active microbes in soil). You could add this points here.

L118: could lead to better projections of... Also, what do you mean by “better”? e.g. increasing realism in a model does not necessarily increase certainty (Sulman et al., 2018. Biogeochemistry)

L124: No explicit question, hypotheses and/or goals? It would be very interesting and useful to know what where the authors expecting, and why, before collecting their data. If there were no hypotheses driving this study, it would be too late now to formulate them.

L129-124: This paragraph seems like a good place to mention previous attempts to include microbial dormancy into process-based biogeochemistry model - other than in MIC-TEM (He at al., 2015) – e.g. in MEND (Wang et al., 2015. ISME) and CORPSE (Salazar et al., 2018. SBB)

L130-133: “First, we describe...” (first person, present tense) “Second, parameterization and validation of... have been shown” (third person) “Third, we applied...” (first

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person, past tense) Unify

L138: I don't understand the use of citation “(Zhuang et al., 2001, 2002, 2003)” here.

L148: in the new model (Figure 1), which was ignored in MIC-TEM

L155: In MIC-TEM-dormancy... We already know that this is the new model (L122 and 128)

L170: represents microbial assimilation...

L173: is maintenance weight

L173-174: “Here.. quality” odd. Maybe ...biomass. CNsoil and CNmic account for substrate quality

L174: Φ is the substrate saturation level

L183: Why is the equation for Dliq not numbered?

L186: Replace “We used... denotes” for “Where... denote”, and move sentence to L191, after equations 7 and 8

L191: “Dormancy rate is affected by substrate availability (Ba, Bd)” this is very confusing. Ba and Bd are active and dormant biomass! (L193 and 202)

L203: Dormant microbes are tough but, no death?

L223: “is different” vague.

L226: RH already defined in L150.

L227: Consider replacing “will also be affected” by: can too, or something like that.

L249: Again, RH already defined in L150.

L250: Because of limitations in the amount of available RH data

L256: See comment in L155

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L279: “were also used. In our model, we assumed” Constant jumps between active and passive voice. Choose one and stick to it. I’d recommend active voice.

L295: CUE was already defined in L152. Also, what do you mean with “was much higher in tundra types than in forests” in Figure 3? None of the boxplots in Figure 3 has CUE on the y axis.

L298: What do you mean by “much higher”? Did you test if those differences are statistically significant? At first glance, it doesn’t seem so.

L299: Similar than the previous comment, what do you mean with “The opposite can be seen from parameter beta”? Although there are no names in the x-axis in the boxplot for beta in Figure 3 (see comment in L841), at first glance there seem to be no (statistically significant) difference in beta between these three sites. Again, did you test this?

L305: Which statistical analysis? There is no mention of any statistical analysis in the methods.

L308: Delete “which is no dormancy-based”. We have this clear by now!

L310: “Another set of sites. . .” So, data from Figure 6 is not the same than from Figure 5? Why using “other set of sites”? Which sites?

L311: “The comparisons between monthly observed RH and simulated RH from two contrasting models were conductd” delete. Redundant. If you decide to keep it, fix typo: “conductd”.

L313: root mean square error (RMSE)

L316: RH already defined in L150.

L317: “This difference further affects soil available nitrogen dynamics, influencing nitrogen uptake by plants, the rate of photosynthesis and NPP.” How? Any result or reference to support this claim?

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L322: “Here positive values of NEP represent sinks of CO₂ into terrestrial ecosystems, while negative values represent sources of CO₂ to the atmosphere.” Already mentioned in L286-288.

L327: Consider deleting “which estimates 75.9 Pg more carbon sink than MIC-TEM does but with less interannual variation (Figure 7c)”. 1. Values for this and previous two sentences are not shown in Figure 7c; 2. Makes the entire sentence long and confusing.

L331: “except a slight decrease during the 1960s (Figure 7)” I don’t see this.

L332: “MIC-TEM-dormancy estimated NPP and RH at 7.94 Pg C yr⁻¹ and 6.4 Pg C yr⁻¹ 333, which are 5.8% and 16.3% less than the estimations from MIC-TEM, respectively (Figures 7a and 7b)” Interesting!

L334: “This pronounced difference of NEP between two models comes from the disparity between the simulated NPP and RH with them.” Confusing

L338: “work for soil decomposition” odd. Maybe: can decompose organic matter.

L351: Consider deleting this “This is because higher RH can cause higher NPP due to the reasons we have mentioned above.”

L353: “since it’s the difference between NPP and RH” either delete or move to introduction or methods (depending on how you frame it).

L355-358: Redundant with L343-349. Combine/synthesize them.

L361: “predicted that the sink is 129.9 Pg” change for something like: predicts a net C sequestration of 129.9 Pg by the end of this century. Same comment for “estimates the sink is 79.5”. Also, note the inconsistency in the use of tenses: “predicted” (past), “estimates” (present)

L365: Change “MIC-TEM but with” for “MIC-TEM, with”.

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L365: Consider adding: under this scenario, “both models. . .”

L370: Start new paragraph with “Under the RCP 2.6 scenario. . .”

L375: an interannual

L381: “Similar seasonal cycle pattern appears for NPP projection”. Not quite so. NPP is the same in winter with or without dormancy, and in the late summer is higher with than without dormancy (i.e. opposite to RH), especially in the RCP 8.5 scenario.

L384: “but similar NEP in other months to MIC-TEM (Figure 10).” Estimation of NEP from January to April seem lower with than without dormancy.

L388: Seems more like the opening of an introduction than of a discussion.

L390: is currently stored? Also, delete “region” or rephrase (e.g. latitude regions).

L391: “climate over this region has warmed in recent decades” other regions are warming too! Maybe you want to say that the magnitude of the warming in these regions is larger, almost twice, that of the global average.

L398: This seems like a good place to discuss your results in comparison with “results from other process-based models”. e.g. - Estimations based on models without dormancy could fit observations of RH as well as estimations with dormancy, but at the cost of underestimating microbial biomass (i.e. the right result for the wrong reasons) (Wang et al., 2014. ISME) - Effects of warming on RH (and microbial biomass) may depend on factors not explicitly considered in your study e.g. differences in predicted RH with and without dormancy increase with temperature AND with the length of the dry periods between wetting events (Salazar et al., 2018. SBB).

L406: What do you mean by “most important microbial activities”.

L413-419: Delete. Repeated information presented in results section.

L423: What do you mean by “and proportion”?

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L427. Delete “Our regional estimate of NEP during the 20th century by MIC-TEM-dormancy is 1.54 Pg C yr⁻¹, and is 0.78 Pg C yr⁻¹ by MIC-TEM.” Repeated information presented in results section.

L428-432: Replace “Schimel et al. (2001) reported that a range of estimates of the northern extratropical NEP is from 0.6 to 2.3 PgC yr⁻¹ in the 1980s. In comparison with our estimates of 1.61 Pg C yr⁻¹ 430 with MIC-TEM-dormancy and 0.84 Pg C yr⁻¹ with MIC-TEM, our regional estimates of NEP are in reasonable range.” with something like: our estimates of. . . are within ranges reported in the literature for northern regions (estimates; Schimel).

L432: “our predicted trend of NEP is very similar to the finding of White et al. (2000), indicating that NEP increases from the 2000s to the 2070s, and then decreases in the 2090s” Which trend? “Trends” in your simulations are very different between models and between RCP scenarios!

L434: “future simulations”? Delete “future”

L434: Delete “future simulations under two contrasting climate scenarios (RCP 2.6 and RCP 8.5) exhibit a large difference of 81.1 Pg C of cumulative NEP during the 21st century by MIC-TEM, but only 6.3 Pg C of that by MIC-TEM-dormancy.” Again, these are results, not a discussion. Also, “future simulations”?

L439: “no dormancy model (MIC-TEM” At this point we know this very well! No need to explain again that MIC-TEM is the “no dormancy model”

L448: “Recent studies have found the capacity of the microbial community to maintain the warming-induced elevated respiration could decrease over time because of acclimation” I don’t understand this sentence.

L451: You talk about “community composition” (L442-446), move on to acclimation (L447-451), and then go back to “community composition” (L451-460). Rearrange this paragraph to avoid this jumping back.

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L461: "above model limitations"? Do you mean: model limitations mentioned above?

L473: Include somewhere in the discussion: How does your work contribute to previous efforts to include microbial dormancy in soil biogeochemistry models (others than MIC-TEM; e.g. MEND, Wang et al., 2015. ISME; CORPSE, Salazar et al., 2018. SBB)? Consistencies/inconsistencies between your "dormancy model" versus other "dormancy models"?

L848: Consider deleting Table 5 and figure 6 and show summary of stats here, as you do in Figure 2

L854: Consider deleting Table 5 and figure 6 and show summary of stats here, as you do in Figure 2

L859: If you decide to keep this figure, consider deleting Table 5 and show stat data (e.g. R2) here.

Technical corrections

L123: In some places the font seems smaller than in the rest of the ms e.g. in L123: "in the Arctic terrestrial ecosystems (north 45 °N above)".

L173: ratios

L309: "Both" instead of "two"

L314: regions

L339: overestimation

L343: "both two" delete "two"

L577: Wang, G. . . (and move down in the ref. list)

L841: x-axis in right boxes

L863: Close parentheses in y axes

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